

# THE CHOLERA;

WHAT HAS IT TAUGHT US?

ITS VARIOUS MODES OF

TREATMENT EXAMINED;

AND

THE MODE BY CALOMEL

SHOWN TO BE THE BEST.

WITH FULL REPORT OF NUMEROUS CASES.

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BY

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## P R E F A C E.

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THE principle portion of this little Essay has already been published at intervals (but necessarily, in a very brief, condensed, and imperfect form) in the pages of the *Lancet*.

I now venture offering it to the notice of my professional brethren, not as a treatise on the nature and pathology of the disease in question, nor as a cherished theory or offspring of my imagination ; but as a plain statement of observation in practice, and facts of treatment ; and an humble endeavour, (which it is hoped may be deemed rational) to deduce therefrom a true *methodus medendi*.

W. J. C.

*Bridgend, Jan. 1850.*



THE  
TREATMENT OF CHOLERA.

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THE fearful epidemic that, during the past year, has for so long a period dwelt among us, making so many hearths desolate, homes tenantless, and spreading far and wide a consternation unparalleled since the days of the Athenian plague (so graphically depicted in the page of Thucydides) has at length subsided; and, although not yet entirely vanished, has nevertheless so far diminished its weekly number of victims, as no longer to occupy with intense and painful interest the public mind.

But whilst we are earnest in grateful acknowledgements to the Giver of Good, for the decrease, and, haply, the removal of a scourge so terrific, a solemn thought, suggestive of a question of vital import, flashes across our minds; and we ask, almost involuntarily, and with a gloomy forboding of evil yet to come,—Is the pest indeed removed from our land, or is it still in the midst of us, hovering in the atmosphere of our dwellings, over our thresholds, by our fire-sides, by our couches, only awaiting the advent of another summer, to burst forth anew and with redoubled virulence to its remorseless work of death?—Is the present calm but a temporary lull, heralding another tornado, more fierce and desolating than the one that has recently swept over us? These questions it is not in human power to solve. Time alone can reveal their answer.

Living however under the shadow of this fear with regard to the future, it behoves us, at all events, to take advantage of and profit as much as possible by the truce now happily granted us, to muster our forces, to provide ammunition, to collect our strength, that we may be better prepared to meet the foe in the battle, which the councils of Heaven may have decreed is yet to be fought; and, surely, above all, it is imperative upon us, the members of the medical profession, to whom the public look for aid with expectant eye, that we should now busily set to work, and casting aside all idle speculations, all petty discords, and rankling jealousies, endeavour to ascertain what the career of the disease has taught us, during the past year, and also during its former visitation in 1832; what we have learned of its nature, its causes, and, above all, of its proper treatment; what particular system, what class of agents, or especial remedy, has best stood the test of experience, and established the fairest and most decided claim to preference. This can only be done justly by collecting, classifying, and contrasting a large number of cases, treated in various ways and according to different systems. The publication of one or two cases, successfully treated by this or that remedy, does not advance our investigations, or add an iota to our therapeutic knowledge. It is not fair to judge from such reports of the efficacy of the means put in force. The inference cannot be truly logical, the data being so deficient. It is from the exhibition of the remedial agent in the same manner and doses, and in a number of cases, presenting as nearly the same symptoms as possible one to another, that we can safely and legitimately draw a deduction of its real worth. The pages of the *Lancet* and other periodicals (professional and otherwise), have been frequently occupied with reports of solitary cases of cholera, treated by the writers of these papers in every imaginable manner, and by every sort of remedy known and unknown ('till now). A heterogeneous mass of ill-ascertained facts and doubtful dogmas, that cannot weigh as real evidence. Had the authors of such papers patiently waited until they (or others



having greater opportunities and a more extensive field of observation) had amply tested their vaunted remedies, in a sufficient number of instances, wherefrom, to judge fairly of their value, I will venture to assert not one-tenth of these productions would ever have met the public eye in print.

My object in presenting this little sketch of a highly important subject to the notice of the profession, is not so much with a view to advocate a certain system, and to record the results of my own experience, as to investigate the merits of the principle modes of treatment that have been from time to time recommended; and to indicate that, to which reason (as I shall endeavour to prove), as well as the test of trial in the long run, point as being the best and safest.

As regards the causes of cholera, predisposing and exciting, I shall not now offer any remarks. This part of the matter, grand and of vast import though it be, I leave to more philosophic heads, and to those whose especial forte is such abstruse investigations. In its actual treatment, I was, during the visitation of the past year, busily engaged; and it is of its treatment alone, founded, I conceive, upon a comprehension of its real nature, derived from a correct appreciation of its characteristic symptoms, that I intend to speak. Whether or no the highly interesting discovery, recently made by Dr. Swayne, be hereafter confirmed by further research, will not, I apprehend, materially influence our rules of treatment. In the pathognomic signs, I contend, if carefully observed and appreciated, we shall not be deceived; and these, alone, must guide us in our curative measures. The knowledge of the existence of those microscopic spores, may teach us haply one day means of prophylaxis—(God grant it!)—but as long as any victims are attacked, can never supply any therapeutic indications.

Before giving the result of my own pathological observations of the malady, and experience of different modes of treatment, it may be as well to state that the plan conceived and adopted by Dr. Ayre, of Hull, (of the administration of small and re-

peated doses of calomel), is the foundation and most prominent part of the method I have successfully pursued in practice, and feel, therefore, bound to recommend. It may not be amiss, also, to say, that so far from having always been a blind and zealous follower of that gentleman's doctrines, that I was at first strongly prejudiced against them, through the representations of numerous professional friends; and was lately impressed with a conviction of their true worth, only after a long course of patient clinical analysis, and after witnessing both in my own practice and that of my friends, the lamentable failure of other plans.

When the mind has to divest itself of preconceived error, and to unlearn what it had before acquired, previous to its being taught the truth, its progress is for the most part slow, but sure. Step by step is secured, by conviction and proof the most positive, of which the arguments are susceptible. Wandering in a labyrinth of error, but deeming ourselves in the right track, we do not jump at the truth when at length it strikes upon us—for we do not for a time regard it as a truth—and illogically hasten to undue conclusions or inferences, as we should be inclined to do, were our minds, with regard to the matter in hand, in the condition of a virgin sheet. For this reason, a convert from another sect will ever be a more zealous and profound advocate—a stancher supporter of his newly-embraced doctrines—than one reared from childhood in the faith. His reason has convinced him, and his belief is based on his understanding.

The treatment of disease to be successful, must be rational. To this proposition may be objected, that the treatment of ague, of syphilis, of neuralgia, &c., is very successful, although confessedly empirical. The reply is:—It cannot be said to be *irrational*. We are as yet in the dark with regard to the specific mode of action of mercury, quinine, iodine, and other remedies: but were these hidden things to be revealed, the propriety of the administration of such agents would doubtless be apparent upon principles resting on a strictly logical basis.



Such I contend to be the case with respect to the calomel treatment of Cholera. It is practically successful because rational. Other plans are failures—not necessarily because they are empirical only—but, because they are irrational and can be proved to be so, and repugnant to the established principles of physic.

Now, my own views respecting the nature of Malignant Cholera are as follows. They are not the fruits of airy speculations or baseless theoretic dreams, but of unprejudiced observation, and sedulous attention at the bedside of suffering. These conclusions I arrange in a tabular form for their better comprehension and effect. The principles of treatment, therefrom deduced, will properly follow :—

1. That Asiatic (or Malignant) Cholera is to be considered in the light of a distinct disease ; and, not merely as an aggravated form of the malady called by some “English Cholera,” and, by others “Serous Diarrhœa ;” having nothing indeed in common with that disease, but some of the symptoms of its earliest stage before collapse is established.

2. That the Cholera virus acts primarily upon the mucous lining membrane of the intestinal canal (chiefly the arch of the colon and lower part of the ileum), producing at first a high degree of irritation, and afterwards a *specific* inflammation, rapidly tending to disorganization. This is proved, I think, by the condition of the villous coat, discovered in those who die early in the collapse.

3. That the ganglia of the organic nerve (semilunar and abdominal) situated within the abdominal cavity, and chiefly supplying the intestinal surface, become unusually excited, and acquire an abnormal exaltation of function. This is shown by their positive condition and by the results of their increased action, described in the next clause.

4. As a consequence of this excessive energy of the sympathetic ganglionic centres, the intestinal canal becomes highly positive—rapid decomposition of the tissues takes place—and

great increase of temperature occurs—arising necessarily from the unwonted activity of the chemical processes of oxidation and destruction. Congestion and semi-paralysis of the contiguous viscera (liver, hepatic-duets, spleen, kidney, &c., but especially the two first-mentioned) soon happens, and becoming severe and persistent ends in—

5. Paralysis, partial, or, in rapidly fatal cases, often complete, of the mesocephalon, medulla oblongata, and upper part of the medulla spinalis; caused directly by the carbonized blood circulating through the nervous centres.

6. The respiration failing (partly the consequence of the extreme prostration of the cerebro-spinal axes, and partly the result of the unusual relative amount of carbon from the portal system, in the circulating fluid), and with it of course the arterial pulse, and the processes whereby the equilibrium of animal heat is maintained; and the heat itself generated, readily explains the great and rapid fall of temperature observed in the extremities, skin, breath, &c.

7. The gastro-pulmonary mucous membrane and the skin—especially of the exposed surfaces—is in an intensely negative condition. This was first, I believe, pointed out by Mr. Blacklock, in his excellent pamphlet. [See *Lancet*, vol. i. 1849, No. 1.] The gastric membrane is secreting free alkali.

8. That paralysis of the kidney, and consequent suppression of urine, is *by no means a constant* or vitally-important symptom; for, in many fatal cases of the most malignant type, I have known urine passed through the *whole course* of the disease, and shortly before dissolution. Moreover, in other instances, where there was total and long-continued suppression, the patients did well. Finally, I have never, in the like cases, detected the presence of urea in the breath, or perspiration, or in the central ventricles or vessels after death. I should not dwell on this point, but that I think much stress has been unduly laid thereon.

9. That it is of very little consideration, to restore the func-

tion of the skin when in abeyance. This, however, has been abundantly urged by many authorities. Much valuable time has often been lost by such useless dallying.

10. Where the collapse is survived, the greatest dangers after reaction is established, consist in the lighting-up of gastritis and encephalitis, terminating in ulceration of the stomach, and effusion beneath the arachnoid.

11. That Cholera has a manifest and remarkable tendency to a periodical return of the accession of its symptoms. Patients frequently recover from the first shock of collapse, which generally sets in at night,—reaction becomes well-established during the day, to be succeeded by a fresh attack of the worst symptoms of prostration after sun-set. This periodicity is noticed, more or less, in all febrile affections.

12. That the disease is certainly contagious, I will venture to say from my own limited experience, it is more contagious than continued fever.

From the foregoing observations and inferences, I draw the following conclusions as rules of treatment. These for like reasons are also presented in a numerical form:—

1. That the remedies ordinarily exhibited with advantage in the treatment of “Cholera Anglica,” and “Diarrhœa Serosa,” are not in general applicable to the treatment of the Asiatic disease; or, at all events, will not suffice for its cure. It being only in the first stage (diarrhœa and vomiting with or without cramps) that this class of remedies are admissible, and then only with great caution. Opium is *never serviceable* in this or any other stage, for reasons hereinafter detailed, although it is our chief reliance in ordinary Cholera.

2. That heat applied in any form to the abdominal parietes, must prove injurious; as tending in a great degree to keep up the high action already going on within, and to accelerate and hurry on to still greater rapidity the destructive processes of inflammation and decomposition. Moreover, by the direct application of caloric, we shall be contributing to maintain the over-



throw of equilibrium in the ganglionic centres, and exciting the galvanic battery, as it were, to increased activity, as insisted on by Mr. Blacklock. [See *Lancet* as above]. I have never witnessed the application of heat to the abdominal region, during the collapse of Cholera, with any other effect than to intensify the condition of prostration, and to increase, or even re-excite vomiting.

3. That for similar reasons, and as an extension of the principle just stated, the artificial reduction of the temperature, over this part of the body, is highly advantageous. The best mode of obtaining this object, is, undoubtedly, by the application of ice constantly renewed; and, also, by its administration "*per orem*." This is almost invariably attended with good effects. It directly lowers the excessive energy of the abdominal ganglia—reduces the temperature within—checks the decomposition of the tissues and other morbid processes—and greatly diminishes cramps, sickness, and irritability of the stomach—thereby enabling that viscus to retain more active and decided remedies. It is astonishing with what greediness the patients will devour it, and how their sunken eyes glisten at its mere aspect.

4. That an agent seems imperatively called for to thoroughly rouse the liver to action—restore the suspended peristaltic function of the intestinal canal—quicken the paralysed gall-duets—and to excite the cerebro-spinal axis to shake off its fatal apathy. *Calomel* is, I conceive, the *only remedy* capable of fulfilling these essential indications; and on which our chief reliance must be placed. Of the general principles of its administration, I shall not venture to say anything. The researches of Michea have placed beyond a doubt its certain and specific action on the liver when given in repeated doses. It should be given, during collapse, in accordance with the system of Dr. Ayre, viz., in "small and repeated doses;" save in the exceptional instance, where violent cramp is the most prominent symptom. It is of very questionable efficacy when given in large doses; because, firstly, it is seldom thus retained on the stomach: and, secondly,

it acts as a powerful sedative to the cerebro-spinal centres, an effect, above all others, to be avoided. The advantages of Dr. Ayre's plan are great and numerous; and fully borne out by facts, as I shall presently show. Calomel, in small doses, acts as a stimulant; and, especially when aided by the important adjuncts—ice, sponging, &c,—is generally retained on the stomach; a very important consideration. Another very useful means of exciting the hepatic viscera is, by mercurial inunction over the right hypochondrium. I have found it very efficacious, but have not tested its value in a sufficient number of cases, to be enabled to speak of it in the same high terms as of the calomel treatment. Dr. Ayre's system, indeed, appears to me, from the inferences of reason, from the records of evidence, and from the results of my own experience, the SHEET ANCHOR—the pillar of strength in our treatment of this most fatal malady of modern times.

5. That the cerebro-spinal centres being in a state of extreme prostration, during the collapse, the administration of opium must be, not only useless, but *manifestly contra-indicated*, as being fraught with great danger. Indeed, I consider this drug TOTALLY INADMISSABLE in any stage, or in any dose. It is *quite powerless* to check the vomiting and purging, or to relieve the cramps. Moreover, it must, by its well-known influence on that organ, greatly promote the risk of congestion of the brain, to which, and its fatal results, there is naturally a strong tendency when reaction takes place. As will be shown in my tabular report of cases, in a subsequent page, its use, in my hands at least, has always been attended with disastrous results. It may be viewed in the light of a mere assertion, but I cannot help saying, that I never saw *in a single instance* the vomiting, even temporarily, checked by its administration, or any effect produced but to do injury. It seems to me to act like an atmosphere of carbonic acid gas to the waning, and all but extinguished, taper of life. It certainly increases the failure of the pulse. Indeed, I perfectly coincide with Mr. Blacklock, in designating opium as a *poison* in Asiatic Cholera.

6. That it is productive of much benefit to sponge the face, neck, chest, and arms, freely and frequently with cold water, slightly acidulated with nitro-hydro-chloric acid. The proportions I have been in the habit of using, are—

Nitric acid . . . . . 2 drachms

Hydro-chloric acid . . . 3 drachms

Mix and add to three quarts of water. The effect of this is to restore the static equilibrium of the nervous centres. The patients like it—request to have it repeated—and say they breathe more freely after its application. It is also advantageous at the same time to apply heat (hot bricks, bags of sand, &c.) to the extremities—but to the extremities alone.

7. That an important auxiliary consists in the administration of sulphuric acid. To me belongs, I believe, the merit (if merit it be) of having first suggested and tried this remedy, and proved its value. The acid answers two great purposes. 1. It neutralizes the free alkali secreted by the stomach, and so checking the vomiting. 2. It tends to destroy electrically the intensely negative condition of the gastric membrane. It is best given in rice-water, ice cold.

8. That the exhibition of powerful diuretics, as is frequently practised, is hazardous and uncalled for. The function of the kidney will generally be restored, with the other suspended secretions, on the establishment of reaction, without any special treatment being thereto directed.

9. That the hot-air bath, and other much-vaunted remedies of the same class, used with the view of promoting diaphoresis, are of no service; and, therefore, indirectly injurious, as in trusting to their presumed efficacy, other means are not put in force; and their employment in this manner induces the loss of time never to be regained. It is not unusual to find a copious sweat bedewing the whole body of the sufferer from first to last. Of what use then is attempting to excite the skin? It is true the hot-air bath in many cases temporarily restores the heat of surface, but it never aids in bringing back the pulse; and, more-



over, is always bitterly complained of by the patients, as adding to their sufferings.

10. That stimulants, if not altogether useless and inadmissible, in the stage of collapse, must be used with the *utmost caution and moderation*, as they may, and frequently do, cause irremediable future mischief, by lighting up gastritis, and increasing the congestion in the cerebral vessels. It will be seen in my report, how very lamentable the result of their employment has proved. Many lives have undoubtedly been lost from their profuse and reckless administration. When one failed, another still more energetic and violent was sought for, with the same utter disregard and ignorance of rational principles of treatment as characterized the views of those, who regard the disease under question only as an exalted form of English Cholera. The theory upon which they are recommended in the collapse of Asiatic Cholera, is, I think, altogether a delusion. It is only during the premonitory stage, that they can be given with prospect of advantage. When the nervous centres are struggling with the morbid impression, that, rapidly gaining strength, threatens them with speedy annihilation of function, stimulants judiciously administered may be of service in aiding them to overcome and shake it off.

11. That whilst keeping in view a salutary caution against the chance of unnecessary salivation, we must not be too hasty in ordering the cessation of our remedies, nor must we at all relax the most rigid rules of regimen. We must be on the watch. Moreover, our prognosis should be well guarded,—the friends being informed of the periodical tendency of the disease.

12. That it is our duty to instruct our patients' friends of the contagious nature of the malady—of course, without exciting needless terror. Much evil has been done by the utter neglect of all means calculated to arrest the spread of the pest, arising from the bold and too-confident assertions of its non-contagiousness put forth by many of our brethren. Certainly, whatever may be our opinions, it is better to err on the safe side.

To sum up then the list of remedies, I think we are warranted in concluding, are the most worthy of trust as being the most rational, and in practice successful. The order of precedence, in which they are placed, indicates what I conceive to be their relative merit. They should, however, be used simultaneously. We are not, I think, justified in trusting alone to any one remedy, when we have to cope with so terrible an enemy; but should use the combined force of all such auxiliaries as have stood the test of experience. 1. CALOMEL: in "small and repeated doses," as recommended by Dr. Ayre; except in such instances wherein, as before stated, violent cramp is present, when it seems preferable to exhibit it in larger doses. [See the report of my own cases]. The only modifications I have adopted and ventured to suggest, in Dr. Ayre's mode of administering this grand remedy, are, *first*, to give it in the form of powder, instead of in pills; for its action is more rapid when so administered. *Secondly*, To give it alone, or at all events not in combination with opium. Dr. Ayre, I believe, now concedes this point. 2. ICE: externally to the abdominal region, and also by mouth. 3. SPONGING with very dilute nitro-hydrochloric acid. 4. SULPHURIC ACID. 5. MERCURIAL INUNCTION of the right hypochondrium. (Strong blue ointment half an ounce to an ounce, repeated in two hours if deemed necessary). Of this list, No. 1. *is certainly, I boldly say, A. 1.* I shall presently adduce statistical evidence of their actual value as well of their relative value, when compared with other remedies.

Let me now briefly consider the objections that have been made to the calomel treatment of cholera. And here I may observe, that no one who has perused the columns of the *Lancet*, and other medical journals, during the last nine months can fail, I should imagine, to be struck with the very small amount of opposition that has appeared to the calomel treatment, as compared with the immense bulk of testimony in its support. And of this little, so great a proportion has emanated from party

feeling, and from persons apparently actuated by vindictive motives, and has consisted of mere speculative hypothesis, unsupported by evidence, as to be unworthy of consideration in the light of real objection. I find scarcely any *positive* testimony to its prejudice. Unfortunately, of the great number who have voluntarily borne testimony in its favour, from time to time, very few indeed have furnished us with statistics. These scattered fragments I have endeavoured to collect. Practitioners have, for the most part contented themselves with assuring us, that they have found calomel treatment eminently successful.

It has, however, been urged by a few against the exhibition of calomel in small and frequently repeated doses :—

1. That it often signally fails in subduing the disease and preserving life.

2. That deluding us into perfect reliance on its powers, and being really useless, it prevents the use of more efficacious remedies.

3. That it is *irrational* and *empirical*.—And,

4. That its employment is attended with disastrous consequences—profuse salivation, &c. &c.

Now, to these objections, I reply—

To the first : That when an individual unfortunately gets an excessive dose of any poison in his system, whether that poison be vegetable, mineral, or atmospheric, he will, in most cases, die in spite of all antidotes, however judiciously and perseveringly administered. It is certainly as irrational and unjust to urge its occasional failure in cholera, in reproach against the use of calomel, as it would be in the treatment of pneumonia, or pericarditis ; because, in these diseases, notwithstanding its exhibition, the inflammatory action too frequently continues its course unchecked. Who, however, will venture to question the general power of calomel over pneumonia ? Or of tartarized antimony over pleurisy ? Or of quina over intermittent fever ?



Nevertheless, patients labouring under these affections, slip through our hands every day.

To the second: That when any other remedy shall be proved to be *more* efficacious, or even of better promise, I will admit, but not until then, that we are not warranted in reposing our chief trust on the calomel.

To the third: That this objection can be framed only by those who take an erroneous view of the pathology of the disease. I think, indeed, we are justified, logically, and from the test of trial, in the conclusion, that the calomel treatment is the most rational of any hitherto broached.

And, to the fourth: That this argument seems scarcely worthy a reply. It is utterly also without foundation. And, suppose it true, is not an occasional sore mouth, or even severe pyalism, fully compensated for in the saving of life? Is a few days' inconvenience or suffering to be thought of, when the alternative is the grave? But this as a result is by no means universal, not even a common consequence. It will be seen by the tables in a subsequent page, that severe salivation seldom occurred either in my own practice, or in the much more extensive experience of Dr. Ayre himself. And, I think when happening, it is generally to be considered as not unavoidable, but arising from want of judgment and caution, in the management of the case.

It has also been mentioned as a circumstance detracting from the merit of Dr. Ayre's treatment, that the proportion of deaths, during the late epidemic, was greater at Hull than elsewhere. A satisfactory explanation, however, of this startling and deplorable fact, consists in the statement of the three following well-known circumstances:—1. The epidemic is acknowledged to have there assumed a very malignant type. 2. Dr. Ayre's system was followed by a few only of the Hull practitioners, who seem to have been sadly divided amongst themselves. 3. The medical staff (as allowed by Dr. Sandforth, see *Lancet*, vol. 2, 1849, No. 17) were inefficient, in point of numbers, to the proper fulfillment of their arduous duties.

It may be observed, as worthy of notice, that even the most determined adversaries of Dr. Ayre's system, with but *very few* exceptions, do not deny the beneficial effects of calomel, given in larger doses; and many of them testify positively to its efficacy.

Before I enter upon a statistical inquiry of the results of Dr. Ayre's, my own, and other methods of treatments, perhaps I may be allowed to say, that the suggestions of treatment (i.e., sponging, &c.) contained in the foregoing pages are applicable only to the *second stage* of cholera—that of *prostration or collapse*; at all events, before reaction becomes established. When that takes place, the scene changes, and we must bring new and different forces upon the field. It would be very irrational and truly empirical, to treat the secondary fever with the same remedies that were our chief reliance during the collapse. I shall not enter into a detail of the remedies now required. We must, in their choice, be entirely guided by general principles, and by the symptoms of each individual case. I may just superficially mention, that counter-irritation over the loins, epigastrium, and abdomen, the exhibition of salines and effervescent, with sulphuric and hydro-cyanic acids, have often proved highly serviceable. Opium is for the most part as strongly contra-indicated in the stage of reaction as in that of collapse.

And now let us look at the evidence.—

In the excellent little work, published by Dr. Ayre in 1833, he gives a detailed history of 219 cases, treated according to his plan. Of these 176 recovered. Only 22 suffered from subsequent soreness of mouth, and but *one* from actual ptyalism. Dr. Ayre's report of the 26th October, 1849, to the Sanitary Committee of the Poor-law Guardians of the Hull Union, gives us the following important and satisfactory results of the treatment pursued by the medical staff under his superintendence, during the visitation of the past season :—

Cases of Diarrhoeal Cholera	-	-	-	-	2906
Cases of Cholera in full collapse	-	-	-	-	725
Cases of impending collapse	-	-	-	-	133
Total	-	-	-	-	3764
Deaths from Diarrhoea	-	-	-	-	6
Deaths from Collapse	-	-	-	-	365
Total	-	-	-	-	371
Recoveries	-	-	-	-	3393

Further on in his report, he says, "It is in the performance of a duty imposed on me that I now bring under your notice the return made by the Registrar-general of the number of deaths which took place in the two parishes where our attendance was given; and from which it will be seen, that notwithstanding the very large proportion of the laboring classes that came under our care, the deaths that occurred with us were 371, whilst the losses sustained by others were 956; in fact, whilst the numbers of our patients were at least two-thirds greater, the deaths with us were nearly two-thirds less than those of others, whose rule it was not to adopt our treatment."

Such is the Dr.'s quiet and ample refutation of the calumnies showered upon him by those of his professional brethren at Hull, who disdaining all trial, and yet confessing themselves at sea, boldly and publicly denounced his mode of treatment as injurious and even murderous. To take then the aggregate result of Dr. Ayre's own experience of the success of his system:—

Cases treated by him in 1832	-	-	-	-	219
Cases treated by him and colleagues in 1849	-	-	-	-	3764
Total	-	-	-	-	3983
Deaths in 1832	-	-	-	-	43
Deaths in 1849	-	-	-	-	371
					414
Recoveries	-	-	-	-	3569

This makes the deaths about 12 or 13 per cent. Such facts may surely be permitted to speak for themselves, without additional remark. Let us therefore go on with the testimony of others. Mr. Wilson, of Sheffield, furnished to Dr. Ayre, in 1833, an account



of the results of his practice. Of 103 cases of malignant cholera, treated by him, exclusively on Dr. Ayre's system, 23 only perished, and of these four had been long ill of other maladies, and five were much neglected by the nurses. The proportion of deaths then in this gentleman's cases may be fairly estimated as under 20 per cent.

I venture to furnish a more detailed and explicit account of my own cases, supplying the most prominent symptoms and after consequences of treatment, as well as my notes will allow. They are as follow :—

TABLE OF CASES ATTENDED BY THE AUTHOR.

No	Sex	Age.	Prominent Symptoms.	Treatment.	Result.	Remarks.
1	male	26	collapse	stimulants and opium	recovery	severe fever
2	do.	34	do.	do.	do.	gastritis
3	do.	40	extreme do.	do.	death	never rallied
4	fem.	11	do.	do.	do.	do.
5	do.	28	do.	do.	do.	died of the fever
6	male	71	do.	do.	do.	do.
7	do.	42	cramps & vomiting	do.	recovery	speedy relief
8	do.	36	do.	do.	death	never rallied
9	fem.	80	extreme collapse	do.	do.	
10	male	61	do.	do.	recovery	
11	do.	38	do.	tartar emetic	death	never rallied
12	fem.	44	do.	do.	do.	do.
13	do.	31	vomiting & collapse	do. and opium	do.	
14	do.	27	do. do.	do.	do.	
15	do.	19	extreme collapse	chloroform and opium	do.	d. of the fever
16	male	16	do.	do.	recovery	violent fever
17	do.	24	vomiting & cramps	do.	do.	
18	do.	4	medium collapse	do.	death	d. of the fever
19	do.	16	do.	brandy and turpentine	do.	never rallied
20	do.	2	do.	do.	do.	do.
21	fem.	39	extreme do.	do.	recovery	severe fever
22	do.	45	do.	do.	death	d. of the fever
23	do.	47	do.	do.	recovery	
24	do.	36	vomiting & cramps	turpentine and opium	death	d. of the fever
25	male	39	do.	do.	do.	
26	do.	54	do.	do.	recovery	
27	do.	39	medium collapse	Ayre's mode	do.	
28	fem.	10	extreme collapse	do.	do.	slow recovery
29	male	41	do.	do.	death	never rallied
30	do.	38	violent cramps	do. and ice	recovery	severe typhalism
31	do.	56	extreme collapse	do. and sulph. acid	do.	slow recovery
32	fem.	21	do.	Ayre's mode	death	d. of abortion
33	male	24	do.	do.	do.	secondary fever
34	do.	41	do.	do. and sulph. acid	recovery	no fever
35	do.	42	medium collapse	do. do.	do.	do.

No.	Sex	age	Prominent Symptoms.	Treatment.	Result.	Remarks.
36	fem.	42	extreme do.	Ayre's mode & sponging	death	much neglect.
37	do.	29	eramps & eollapse	do. do.	recovery	no fever
38	male	46	eramps	do. do.	do.	do.
39	fem.	13	extreme eollapse	do. do.	death	neglected
40	do.	4	do.	do. do.	recovery	slight ptyalism
41	male	12	do.	do. and ice	do.	do.
42	fem.	41	eramps & eollapse	calomel, large doses	do.	rapid reecovery
43	male	54	vomiting & eramps	do.	do.	do.
44	do.	36	do.	do.	do.	do.
45	do.	31	vomiting	Ayre's mode	do.	
46	fem.	35	extreme eollapse	do.	death	never rallied
47	do.	64	eramps & vomiting	calomel, large doses	recovery	rapid recovery
48	do.	41	do. and eollapse	do.	do.	much ptyalism
49	male	52	do. and vomiting	Ayre's mode	do.	speedy relief
50	do.	31	eollapse	do.	do.	slow reecovery
51	do.	41	do.	do.	do.	do.
52	do.	37	do.	do.	do.	do.
53	do.	32	do.	do.	do.	slight ptyalism
54	fem.	4	do.	do. and ice	do.	
55	do.	16	vomiting	do.	do.	
56	do.	29	eollapse	do.	do.	
57	male	2	do.	do.	death	never rallied
58	do.	71	do.	do.	reecovery	
59	do.	43	do.	do.	do.	
60	fem.	84	do.	do. and sulph. acid	death	
61	do.	6	do.	do. do.	reecovery	
62	male	17	violent eramps	calomel, large doses	do.	sore. of mouth
63	do.	24	medium eollapse	Ayre's mode	do.	do.
64	do.	50	do.	do.	do.	
65	fem.	40	violent eramps	calomel, large doses	do.	speedy relief
66	male	30	medium eollapse	Ayre's mode	do.	
67	do.	30	extreme collapse	do., ice, & sulph. acid	do.	slight ptyalism
68	fem.	30	do.	do. do. do.	death	neglected
69	do.	45	do.	do. do. do.	recovery	gradual
70	do.	45	do.	do. do. do.	do.	do.
71	male	25	do.	do. do. do.	do.	do.
72	do.	35	do.	do. do. do.	do.	do.
73	do.	30	do.	do. do. do.	do.	do.
74	do.	35	do.	do., ice, and sponging	do.	severe fever
75	do.	40	do.	do. do. do.	do.	
76	fem.	65	vomiting	do. do. do.	death	
77	male	50	collapse	do. do. do.	recovery	slight ptyalism
78	do.	40	do.	do. do. do.	do.	
79	fem.	30	eramps	calom. large doses & ice	death	d. of the fever
80	do.	4	do.	do.	recovery	no fever
81	do.	15	vomiting	A's mode, ice, & spong.	do.	do.
82	male	42	extreme eollapse	do. do. do.	do.	do.
83	fem.	45	do.	do. do. do.	death	never rallied
84	male	25	do.	do. do. do.	do.	
85	fem.	20	do.	do. do. do.	recovery	no fever
86	male	15	do.	do. do. do.	do.	do.
87	do.	25	do.	do. do. do.	do.	do.
88	do.	71	do.	do. do. do.	do.	do.
89	fem.	40	do.	do. do. do.	death	
90	do.	24	do.	do. do. do.	reecovery	very long ill

## CLASSIFICATION AND ANALYSIS OF THE PRECEDING TABLE.

Cases treated by stimulants and opium	- - 17	Result	- - 10 deaths
Cases treated by stimulants alone	- - - 5	Result	- - 3 deaths
Cases treated by Tartar emetic	- - - 4	Result	- 4 deaths
Cases treated by large doses of calomel	- - 9	Result	- - 1 death
Cases treated on Dr. Ayre's system, modified	55	Result	- - 13 deaths
Total	- - - - 90 cases		31 deaths

I do not deem it requisite to make any further comments on the foregoing table. It is brought forward as a simple and unvarnished statement of facts, in support of the calomel treatment.

To give even an outline of the mass of argument and evidence that has appeared on this subject, in the leading medical journals, would be a very tedious although highly-interesting task. I shall not attempt doing so in this little essay, but refer my readers to the pages of the *Lancet*, the *Medical Times*, and other periodicals, wherein they will find the advocates and supporters of the calomel treatment, far outweighing their adversaries, both in point of number and value of statistical testimony. I will, however, give a brief summary of some of the principal contributions on this important topic.

Drs. Niddrie, Mackay, Searle, and others, whose long experience of the malady in the Eastern climes, peculiarly its own, renders their praise or censure worthy of deep attention, testify most strongly to the efficacy of the calomel treatment. Messrs. Mitchell, Wright, Wilson, Morley, Spong, Popham, Badeley, Monday, and a host of others, practitioners of the highest respectability—gentlemen whose veracity cannot be questioned—many of them also connected with infirmaries and dispensaries, affording them ample field for observation—have voluntarily come forward, and in the strongest language and most earnest manner signified their approbation of Dr. Ayre's mode of treatment.

But, perhaps, the most astounding results appear in the account furnished by Mr. Jones, of the Wandsworth Road, who,

out of upwards of 900 cases of cholera and choleraic diarrhœa, treated by him on Dr. Ayre's system, during the late epidemic, lost only seven. This gentleman in his report states, that he at first (like myself) pursued the saline method, and lost every patient; and, afterwards the stimulant and opiate system, which proved nearly as unsuccessful.

The accounts received from provincial hospitals, are very much in favour of the calomel system. At Newcastle, Sheffield, Liverpool, and Plymouth, where this plan was adopted, the results were very satisfactory. In the two last mentioned places especially, the saline treatment was discarded in order to make room for that of Dr. Ayre's.

It is much to be regretted that Dr. Ayre's treatment was not pursued and given a fair trial, in any of the metropolitan hospitals. The results of treatment, therefore, reported from these establishments, are not of any *positive* value as affording testimony of the efficacy of that mode of treatment: but are valuable only in another point of view, as showing, in most instances, the lamentable failure of other curative measures. In King's College Hospital (according to the reports of the proceedings published in the *Lancet*) the treatment consisted chiefly in the exhibition of powerful astringents in combination with stimulants. The results were most deplorable, notwithstanding that the most assiduous care and attention was lavished on the sufferers. At Charing Cross Hospital, stimulants were principally had recourse to; and the wetted sheet was also put in requisition: the result was equally discouraging. Almost every case of a malignant type, with extreme collapse, was lost. The report from Bartholomew's appeared in the *Times* paper about three months since. It states that ice was the principal remedial agent employed, and gives the result as follows:—

Cases	-	-	-	-	-	478
Deaths	-	-	-	-	-	199
						<hr/>
Recoveries	-	-	-	-	-	279

It is not said, whether or no, calomel was generally given, at the same time with the ice.



From the reports and statistics I have here adduced, and from all I have been enabled to learn from numerous authentic sources, I have arrived at the following general ultimate estimate of the relative value of the calomel treatment, viz.—

Deaths under Dr. Ayre's system, - - - 20 to 24 per cent.

Deaths under other methods of treatment,

saline, stimulant, &c. - - - - - 50 to 70 per cent.

This, I beg *earnestly* to assure my readers, is not a rough and prejudiced guess, but the conviction, resulting from patient inquiry.

Before I finally take leave of the subject, I would wish to say a few words respecting two modes of treatment, which have very much engaged the attention of the profession, and, considering the sources from whence they emanated, certainly are entitled to respect, and a due investigation of their merits. These, are the tartar-emetic (saline) treatment of Dr. Billing, and the sulphur method of Mr. Grove. With regard to the former, I can only say, that in my own hands, and under the management also of many of my friends, it proved a *decided and deplorable failure*—I lost *every case*, and those not of an especial malignity of type—insomuch so, indeed, that nothing would induce me, in the event of a fresh outbreak, again to waste valuable time in giving it a new trial, which, I firmly believe, must prove equally futile: and, I am by no means solitary in the expression of this opinion. I perfectly coincide with the learned and accomplished physician, who has so ably advocated its adoption, in his general view of the pathology of the disease, in his rejection of opiates, and in his principles of treatment; but I cannot agree with his therapeutical views, or, in the opinion, that tartarized antimony is capable of fulfilling the important indications required of it: in this judgement, I respectfully contend, I am amply borne out by established proofs.

Of the sulphur treatment, I have had, unfortunately, no experience. Valuable, as I believe it must prove, as a means of prophylaxis, I cannot conceive it of much efficacy as a remedy.

In my opinion, it is far too inert and powerless an agent to be of much avail when collapse is established. One objection, however, cannot be urged against it. It certainly does not seem to be irrational.

In conclusion, I may be permitted to express a hope, that, should this little pamphlet, meet the eye of any of those of my professional brethren, who have been hitherto led by false representations, or distorted pathological views of the disease, to regard Dr. Ayre's treatment of Asiatic Cholera as lying within the domain of empiricism; its perusal, may induce them to pause ere they condemn without hearing or trial, and calmly examine the arguments and proofs, both of its supporters and its adversaries.

My object is to further the elucidation of truth. Faithful to the precepts I have been taught, I have always followed and courted her for her own sake, irrespective of system and fearless of ridicule. If the future experience of abler minds, should hereafter prove that, in the present instance, I have been pursuing a phantom and a delusion, having merely the outward semblance and garb of the object of my desire, but being all hollow within, our common liability to err, and my earnest wish to add a mite of help in the good cause, must plead my excuse to my readers.





